RESEARCHES

ON THE THEORY OF THE

PRINCIPAL PHÆNOMENA OF PHOTOGRAPHY

IN THE

DAGUERREOTYPE PROCESS.

By A. CLAUDET.

ALTHOUGH the Daguerreotype process has during the last ten years been investigated by a great number of philosophers, and brought to a considerable degree of perfection by a still greater number of practitioners, it may appear surprising that the principal phænomena upon which this new art is founded, are still enveloped in a mysterious darkness.

My constant endeavour has been to explain them, and at the two last meetings of the British Association I have had the honour of communicating the results of some of my researches.

The phænomena which have not yet been satisfactorily explained, and of which I shall have to treat in the present paper, are those referring to the following points:—

1. What is the action of light on the sensitive coating?

2. How does the mercurial vapour produce the Daguerreotype image?

3. Which are the particular rays of light that impart to the chemical surface the affinity for mercury?

4. What is the cause of the difference in achromatic lenses between the visual and photogenic foci? why do they constantly vary?

5. What are the means of measuring the photogenic rays, and of finding the true focus at which they produce the image?

At the last meeting of the British Association, which took place at Swansea, I announced that the decomposition of the chemical surface of the Daguerreotype plate by the action of certain rays of light, produced on that surface a white precipitate, insoluble in the hyposulphite of soda, which, when examined by the microscope, had the appearance of crystals reflecting light, and which, when seen by the naked eye, were the cause of a positive Daguerreotype image.

This fact had not been observed before. The opinion of Daguerre himself and other writers was, that the action of light on the iodide of silver had only the effect of darkening

the surface, and consequently of producing a negative image. But it escaped them, that, under the darkened iodide of silver, another action could take place after a continued exposure to light, and that the hyposulphite of soda washing could disclose a positive image. I have proved this unexpected fact in obtaining, by the action of light only, and without mercury, images having the same appearance as those developed under the action of mercurial vapour. This direct and immediate effect of light is certainly remarkable; but the Daguerreotype process is not founded on that principle on account of the slowness of its action; and it is fortunate that, long before light can produce the white precipitate I have alluded to, it operates another effect, which is the wonderful property of attracting the vapour of mercury. This vapour is condensed in the form of a white powder, having also, when examined by the microscope, the appearance of reflecting crystals. The Daguerreotype image is due to this property, which is the most beautiful feature of Daguerre's discovery.

M. Moser has given an ingenious theory of the action of mercury. Knowing that the yellow ray had the property of continuing the effect commenced by light on the iodide of silver, he has supposed that mercury, when in a state of vapour, evolves a latent yellow light, and to the action of that yellow light of mercurial vapour he ascribes the continuation of the decomposition of the iodide of silver. But as the analysis of the surface discloses the presence of mercury, that metal must have been amalgamated with the silver set free after the action of light. We must therefore look for another explanation of the phænomenon.

It is more probable that light exercises a twofold action on the iodide of silver, whether it is combined or not with chlorine or bromine. By one, the iodide is decomposed, and the silver set free is precipitated on the surface in the form of a white powder or small crystals; by the other, which begins long before the former, the parts affected by light have been endowed with an affinity for mercurial vapour.

By means of my photographometer, to the principle of which I shall presently refer, I have been able to ascertain that the pure light of the sun performs in about two or three seconds the decomposition of the bromo-iodide of silver, which is manifested by the white precipitate; while the same intensity of light determines the affinity for mercurial vapour in the wonderfully short space of about $\frac{1}{1000}$dth part of a second. So that the affinity for mercury is imparted by an intensity of light 3000 times less than that which produces the decomposition manifested by the white precipitate.

For this reason it is difficult to suppose that the two actions

are the same. We must admit that they are different. Long before it can effect the decomposition of the surface, light imparts to the sensitive coating the affinity for mercurial vapour; and this appears to be the principle of the formation of the image in the Daguerreotype process.

In a paper I communicated to the Royal Society on the 17th of June 1847 (see Transactions), and an abstract of which I read before the Association at Oxford, I stated that the red, orange and yellow rays were destroying the action of white light, and that the surface was recovering its former sensitiveness or unaffected state after having been submitted to the action of these rays. I inferred from that curious fact that light could not have decomposed the surface; for if it had, it would be difficult to understand how the red, orange, or yellow rays could combine again, one with another, elements so volatile as bromine and iodine, after they had been once separated from the silver.

But I had not yet been able to ascertain that, when light has decomposed the bromo-iodide of silver, the red, orange or yellow rays cannot restore the surface to its former state. The action of light, which can be destroyed by the red, orange or yellow rays, does not determine the decomposition, which would require an intensity 3000 times greater; it is the kind of action produced by an intensity 3000 times less, giving the affinity for mercury, which is completely destroyed by the red, orange or yellow rays. It seems, therefore, that I was right in saying that there was no decomposition of the compound during the short action which is sufficient to give the affinity for mercury, and in ascribing the formation of the image only to that affinity. White light, or the chemical rays which accompany it, communicate to the surface the affinity for mercury, and the red, orange, or yellow rays withdraw it. I must notice here a singular anomaly; viz. that when the sensitive surface is prepared only with iodine without bromine, the red, orange or yellow rays, instead of destroying the action of white light, continue the effect of decomposition as well as that of affinity for mercury. Still there is a double compound of iodine which is far more sensitive than the simple compound, and on which the red, orange, or yellow rays exercise their destructive action as in the case of the bromo-iodide.

The phænomenon of the continuing action of the red, orange or yellow rays, on the simple compound of iodide of silver, was discovered by M. Ed. Becquerel; and soon after M. Gaudin found, that not only those rays continue the action by which mercury is deposited, but that they develope without mercury an image having the same appearance as that produced by mercurial vapour.

M. Gaudin, not having observed the fact of the white pre-
cipitate, which is the result of the decomposition by the action
of light, could not explain the cause of the image brought out
under the influence of the yellow ray.

I have observed that the iodide of silver without bromine is
about 100 times more sensitive than the bromo-iodide to the
action of light, which produces the decomposition of the com-
pound forming the white precipitate of silver, while it is 100
times less sensitive for the effect which gives the affinity for
mercury. This seems another reason for supposing that the two
actions are different. It may be that, in the case of the iodide
of silver alone, the decomposition being more rapid, and the
affinity for mercury slower than when bromine is added to
the compound, the red, orange, and yellow rays having to
act upon an incipient decomposition, have the power, by their
own photogenic influence, of continuing the decomposition
when it has begun. This may explain the development of
the image under red, orange, or yellow glasses, according to
M. Gaudin's discovery. But in the case of the bromo-iodide
of silver, the red, orange, or yellow rays have to exert their
action on the affinity for mercury, begun a long time before
the decomposition of the compound; and they have the pro-
perty of destroying that affinity.

So that it would appear that all the rays of light have the
property of decomposing the iodide of silver in a longer or
shorter time, as they have that of producing the affinity for
mercury on the bromo-iodide of silver; with the difference,
that on the former compound the separate actions of the
several rays continue each other, and that on the second com-
pound these separate actions destroy each other. We can
understand that, in the first case, all the rays are capable of
operating the same decomposition; and that in the second,
the affinity for mercury when imparted by one ray is destroyed
by another. This would explain the various phænomena of
the formation of the two different deposits I have described,
and also explain the anomaly of the continuation of the action
of light by the red, orange, or yellow rays, according to M.
Ed. Becquerel's discoveries on the iodide of silver; and of the
destruction of that action by the same rays, according to my
own observations on the bromo-iodide of silver.

The red, orange and yellow rays, when acting on an un-
affected surface, are considerably less capable than the most
refrangible rays of imparting the affinity for mercurial vapour
on both the iodide and bromo-iodide of silver; and they de-
stroy that affinity when it has been produced on the bromo-
iodide of silver by the photogenic rays. It follows from this
fact, that when the red, orange, or yellow rays are more abun-

dant in the light than the most refrangible rays, the photo-genic effect is retarded in proportion to the excess of these antagonistic rays. This happens when there exists in the atmosphere some vapours which absorb the most refrangible rays. In these circumstances the light appears rather yellow; but it is very difficult to judge by the eye of the exact colour of the light, and of the proportion of photogenic rays existing in the atmosphere at any given moment.

The vapours of the atmosphere which render the light yel-low, act as any other medium intercepting the blue rays, and those which have the same degree of refrangibility. I prove, by a very simple experiment, the comparative photogenic action of rays which have passed through such media, and of those which have met with no similar obstacle; also that media which intercept the photogenic rays can let pass freely the illuminating rays.

If I cover an engraving one-half with light yellow glass, and place it before my camera obscura in order to represent the whole on a Daguerreotype plate, I find that during the time which has been necessary to obtain the image of the half not covered, not the slightest effect has been produced on the half covered with the yellow glass.

Now if I cover one half with deep blue glass and the other with the same light yellow glass, the engraving will be seen very distinctly through the yellow glass, and not at all through the blue. In representing the whole, as before, on the Da-guerreotype plate, the half which was clearly seen by the eye has produced no effect; and the other, which could not be seen, is as fully represented, and in nearly as short a time, as when no blue glass had been interposed.

Thus we might construct a room lighted only through an inclosure of light yellow glass, in which light would be very dazzling to the eye, and in this room no photographic opera-tion could be performed; or a room inclosed by deep blue glass, which would appear very dark, and in which the pho-tographic operation would be nearly as rapid as it would be in open air.

Thus we may conceive certain states of the atmosphere under which there will be an abundance of illuminating rays, and very few photogenic rays; and some others, under which the reverse will take place.

Considering how difficult it is to judge by the eye alone of the photogenic state of light, we can understand why the photographer is constantly deceived in the effect he tries to produce, having no means to ascertain beforehand, with any degree of certainty, the intensity of light. For these reasons I turned my attention to contrive an apparatus by which

I could test at the same time the sensitiveness of the Daguerreotype plate and the intensity of light.

I succeeded in constructing an instrument which I have called a photographometer, the description of which appeared in the Philosophical Magazine for the month of November 1848.

As I have since improved it considerably, and made with it a great number of experiments, I shall briefly refer to this instrument, and describe the useful alterations I have made.

In the instrument described in the Philosophical Magazine for November 1848, the light struck the Daguerreotype surface during the passage on an inclined plane of a metallic plate having seven apertures in a horizontal line, following the geometrical progression 1, 2, 4, 8, 16, 32, 64; so that the Daguerreotype plate being covered with another metallic plate having four series of seven holes, the effect of light through every one of the seven holes was represented in proportion to the opening of the moveable plate. Every one of the four series of holes indicated the same number of white spots, and the number of spots was the measure of the light at the moment. I had four series of holes in order to try several preparations on the same plate, or to test the light on the same plate at four different times.

The improvement I have made consists in my being able to shut every one of the holes by means of sliding blades; so that I can continue, by repeated falls, the geometrical progression from 1 to 512 on one plate; and when a second plate is added to the double apparatus, from 1 to 8192. This enables me to compare and follow the different effects of light in a considerable range of intensities. This is done in the following manner:—After having given one fall with all the slides open, I shut one and give another fall, then shut the second slide and give two falls, and so on, always doubling the number of falls for every new slide shut.

It is by this means that I have been able to discover at what degree of intensity of light the effect called solarization is produced;—on well-prepared plates of bromo-iodide it does not begin under an intensity 512 times greater than that which determines the first effect of mercury;—and also at what degree the decomposition producing the white precipitate without mercury manifests itself, both on iodide and on bromo-iodide of silver. On the first, it is 100 times quicker than on the bromo-iodide; and on the last, it is produced by an intensity 3000 times greater than that which developes the first affinity for mercury.

The slides enable me to try the effect of different insulated rays on plates affected by white light. This is done by shut-

ting one-half of each hole in pushing the sliding blades just enough for that purpose. In that state I submit the surface acted on by a great number of intensities of light to the subsequent radiation through red, orange, or yellow glasses, or any other coloured transparent media, in order to examine the action of these radiations on one-half of the effects produced by each intensity of light. By these means I have found, that before light has decomposed the surface and produced the white precipitate, the red, orange, and yellow rays destroy the affinity for mercury, and continue it when the decomposition has begun.

In the course of my experiments I noticed a curious fact, which proved very puzzling to me, until I succeeded in assigning a cause to it. I shall mention it here, because it may lead to some further discoveries. I observed that sometimes the spaces under the round holes, which had not been affected by light during the operation of the photographometer in a sufficient degree to determine the deposit of mercury, were, as was to be expected, quite black ; while the spaces surrounding them were in an unaccountable manner slightly affected by mercury. At first I could not explain the phænomenon, except by supposing that the whole plate had been previously by accident slightly affected by light, and that the exposure through the holes to another sort of light had destroyed the former effect. I was naturally led to that explanation, having before observed that one kind of light destroys the effect of another; as, for example, that the effect of the light from the north is destroyed by the light from the south, when certain vapours existing in the latter portion of the atmosphere impart a yellow tint to the light of the sun. But after repeated experiments, taking great care to protect the plate from the least exposure to light, and recollecting some experiments of M. Moser, I found that the affinity for mercury had been imparted to the surface of the Daguerreotype plate by the contact of the metallic plate having the round holes, while the space under the hole had received no similar action. But it must be observed that this phænomenon does not take place every time; some days it is frequent, and in some others it does not manifest itself at all. Considering that the plate furnished with round holes is of copper, and that the Daguerreotype plate is of silver plated on copper, it is probable that the deposit of mercury is due to an electric or galvanic action determined by the contact of the two metals ; and perhaps the circumstance that the action does not take place every time, will lead to the supposition that it is developed by some peculiar electric state of the ambient atmosphere ; and by a degree of dampness in the air, which would increase the

electric current. May we not hope that the conditions being known in which the action is produced, and by availing ourselves of that property, it will be possible to increase on the Daguerreotype plate the action of light? for it is not improbable that the affinity for mercury imparted to the plate is also due to some electrical influence of light. How could we otherwise explain that affinity for mercury given by some rays and withdrawn by some others, long before light has acted as a chemical agent?

Photography is certainly one of the most important discoveries of our age. In relation to physics and chemistry, it has already been the means of elucidating many points which had not been investigated, or which were imperfectly known before. We may certainly expect that its study will prove of considerable use to the progress of these sciences. But it is in reference to optics that it opens a large field for research and discovery. Had Newton been acquainted with the properties with which light is endowed in the phænomena of photography, there is no doubt he would have left a more complete theory of light, and of the various rays which compose it.

Since the discovery of photography, opticians have turned their attention to the constructing of new combinations of lenses, in order to increase the illuminating power without augmenting the aberration of sphericity. It is due to justice to state here, that the optician who first produced the best lenses for photography is M. Voigtlander of Vienna, and they still are the most perfect that a photographer can use, particularly for portraits. In this country an optician of great merit, Mr. A. Ross, has constructed lenses on similar principles; and at all events has succeeded in producing some which work as quick, and give an image as perfect in every respect. In Paris M. Lerebours is renowned for lenses with longer focus, which are better adapted for taking views than any I have tried.

From the beginning of photography it was well known that the effective rays being the most refrangible, had a shorter focus than those producing white light; and for this reason Daguerre himself recommended the use of achromatic lenses, in which all the rays were supposed to coincide nearly at the same focus. All cameræ obscuræ were furnished with achromatic lenses, and constructed so that the plate could be placed exactly at the same distance as the ground glass on which the image had appeared the best defined. But with these cameræ obscuræ it was very difficult to obtain a photographic image so perfect as that seen on the ground glass; and it was only now and then, and as if by accident, that good pictures could be produced.

I soon observed that anomaly, and imagined that it was due to some errors in the respective position of the two frames; one holding the ground glass, and the other containing the plate, which, by warping or some other causes, might have been shifted to different distances from the object-glass.

Not being able to assign another reason for the error, I constructed a camera obscura in which the ground glass and the plate were exactly placed in the same frame. In doing so I hoped to avoid the least error or deviation. But to my surprise, the more I was correct in my adjustment, the less I could obtain a well-defined Daguerreotype picture. This proved to me that I had to seek for another cause of the difficulty; and before going any further, I decided to try if the visual focus did or did not really coincide with the photogenic focus. For the experiment, I placed at a distance from the camera obscura several screens on different planes : these screens being covered with black lines, I could see them very distinctly on the ground glass. I tried the focus on one of the screens. To my surprise and delight, I found that invariably the one which had come out well-defined on the ground glass was confused on the Daguerreotype plate, and *vice versâ*. This was sufficient to prove to me the cause of the difficulty I had been labouring under, viz. that the visual focus had not coincided with the photogenic focus. But the most surprising feature of that discovery was, that the photogenic focus was longer than the visual focus. On first consideration it should have been shorter, as the rays operating in photography are the most refrangible. Although I could not at first understand the cause of this anomaly, it was sufficient for me to know that, in order to have a well-defined Daguerreotype picture, I had only to set the focus on the ground glass for an object nearer the camera at the distance indicated by the experiment with the various screens. Continuing my experiment, I found some lenses in which the photogenic focus was shorter, and some others in which the two coincided.

I communicated a paper on the subject to the Royal Society and to the Académie des Sciences in May 1844, and from that time photographers have been able to find the true photogenic focus of their camera ; and opticians, who at first denied the fact, have at last studied and considered the question, trying to construct lenses in which the two foci should agree.

M. Lerebours of Paris was the first who, on my suggestion, examined the subject; and he communicated a paper to the Académie des Sciences, in which he explained the cause of the difference. He stated that, by altering the proportion

between the angles inscribed in the curves either of the crown- or flint-glass, he could render at will the photogenic focus longer or shorter than the visual focus, and by the same means could bring them to the same point. There is no question that M. Lerebours was right as far as the result referred to the chromatic correction; but if, according to the density of the two glasses, certain curvatures are required to correct the spherical aberrations, these curvatures cannot be altered with impunity only for the purpose of changing the directions of the most refrangible rays. For this reason I have always preferred lenses in which the spherical aberration is the most perfectly corrected, without caring whether the photogenic rays coincided or not with the visual rays, having the means of ascertaining how I could obtain on my Daguerreotype plate the best-defined image. In fact, from my own observation that the red, orange, and yellow rays are antagonistic to the photogenic rays, and that the last rays have a greater power when the former are proportionately less abundant, I am of opinion that when the photogenic rays are only condensed on the plate, and the others are dispersed on the spaces more or less distant from the photogenic points, the action is more rapid. Rapidity being the principal object in photography, I prefer lenses in which the two foci are separated, although the operation is a little more difficult, and requires considerable care.

The question of the photogenic focus is involved in another kind of mystery, which requires some attention. I have found that with the same lenses there exists a constant variation in the distance between the two foci. They are never in the same relation to each other: they are sometimes more or less separate; in some lights they are very distant, and in some others they are very near and even coincide. For this reason I constantly try their position before I operate. I have not been able to discover the cause of that singular phænomenon, but I can state positively that it exists. At first I thought that some variations in the density or dispersive power of the atmosphere might produce the alteration in the distance between the two foci; or that when the yellow rays were more or less abundant, the visual rays were refracted on different points on the axis of the foci, according to the mean refrangibility of the rays composing white light at the moment. But a new experiment has proved to me that these could not be the real causes of the variation. I generally employ two object-glasses; one of shorter focus for small pictures, and the other of longer focus for larger images. In both the photogenic focus is longer than the visual focus; but when they are much separated in one they are less so in the other: sometimes when they coincide in one, they

are very far apart in the other, and sometimes they both coincide. This I have tried every day during the last twelve months, and I have always found the same variations. The density of the atmosphere, or the colour of light, seems to have nothing to do with the phænomenon, otherwise the same cause would produce the same effect in both lenses. I must observe that my daily experiments on my two object-glasses are made at the same moment and at the same distance for each, otherwise any alteration in the focal distance would disperse, more or less, the photogenic rays, which is the case, as it is easy to prove. The lengthening or shortening the focus, according to the distance of the object to be represented, has for effect to modify the achromatism of the lenses. An optician, according to M. Lerebours's calculation, can at will, in the combination of the two glasses composing an achromatic lens, adapt such curvatures or angles in both that the visual focus shall coincide with the photogenic focus; but he can obtain this result only for one length of focus. The moment the distance is altered, the two foci separate, because the visual and photogenic rays must be refracted at different angles in coming out of the lens, in order to meet at the focus given for one distance of the object. If the distance is altered, the focus becomes longer or shorter; and as the angle at which different rays are refracted remains nearly the same, they cannot meet at the new focus, and they form two images. If the visual and photogenic rays were refracted parallel to each other, in coming out of the lens they would always coincide for every focus; but this is not the case. It seems, therefore, impossible that lenses can be constructed in which the two foci will agree for all the various distances, until we have discovered two kinds of glasses, in which the densities or the refractive power will be in the same ratio as their dispersive power.

There is no question so important in photography as that which refers to finding the true photogenic focus of every lens for various distances. I have described the plan I have adopted for that purpose. By means of that very simple instrument, every photographer can always obtain well-defined pictures with any object-glasses. But there is another method of ascertaining the difference between the two foci, which has been lately contrived by Mr. G. Knight of Foster Lane, London. That gentleman has been kind enough to communicate to me the very ingenious and simple apparatus, by which he can at once find the exact difference existing between the visual and photogenic focus, and place the Daguerreotype plate at the point where the photogenic focus exists. I am very glad he has entrusted me with the charge of bringing his invention before the British Association. For the scientific

investigation of the question Mr. Knight's apparatus will be most valuable to the optician, as it will afford him [the means of studying the phænomenon with mathematical accuracy.

Mr. Knight's apparatus consists in a frame having two grooves; one vertical, in which he places the ground glass, and the other forming an angle with the first destined to receive the plate; the planes of the grooves intersect each other in the middle. After having set the focus upon the ground glass, this last is removed, and the plate is placed in the inclined groove. Now if a newspaper or any large printed sheet is put before the camera, the image will be represented on the inclined plate; and it is obvious in its inclination the various points of the plate will meet a different focus. The centre of the plate will coincide with the visual focus; by its inclination it will in one direction meet the photogenic focus at a point more or less distant from the centre, if the photogenic focus is shorter than the visual focus, and in the other direction if it is longer. The frame is furnished with a scale of division, having the zero in the centre. When the image is represented on the Daguerreotype, by applying against it another moveable scale of division similar to the other, the operator can find what is the division above or under zero at which the image seems best defined; and after having removed from the camera the experiment frame, and set the focus as usual on the ground glass, he has only to move the tube of the object-glass by means of the rack and pinion, and to push it in or out, a space corresponding with the division of the scale indicating the deviation of the true photogenic focus : the tube of the object-glass is for that purpose marked with the same scale of division.

In order to enable the members of the Association to judge of the merit of Mr. Knight's invention, I have had his apparatus applied to a small camera with which I made my experiment. By exhibiting at the same time Mr. Knight's method and my own, a comparison of the two may be made, and they will be both better understood.

Before concluding, I shall call the attention of all persons conversant with optics to the singular fact I have observed respecting the constant variation of the two foci. I have not been able yet to find its cause, and I leave its investigation to more competent persons. I hope at the next meeting of the Association we shall know more on the subject.